

January 30, 1873.

GEORGE BUSK, Esq., Vice-President, in the Chair.

The following communications were read :—

- I. “Note on the Origin of *Bacteria*, and on their Relation to the Process of Putrefaction.” By H. CHARLTON BASTIAN, M.D., F.R.S. Received November 20, 1872.

In his now celebrated memoir of 1862, M. Pasteur asserted and claimed to have proved (1) that the putrefaction occurring in certain previously boiled fluids after exposure to the air was due to the contamination of the fluids by *Bacteria*, or their germs, which had before existed in the atmosphere, and (2) that all the organisms found in such fluids have been derived more or less immediately from the reproduction of germs which formerly existed in the atmosphere.

The results of a long series of experiments have convinced me that both these views are untenable.

In the first place, it can be easily shown that living *Bacteria*, or their germs, exist very sparingly in the atmosphere, and that solutions capable of putrefying are not commonly infected from this source.

It has now been very definitely ascertained that certain fluids exist which, after they have been boiled, are incapable of giving birth to *Bacteria*, although they continue to be quite suitable for the support and active multiplication of any such organisms as may have been purposely added to them. Amongst such fluids I may name that now commonly known as “Pasteur’s solution,” and also one which I have myself more commonly used, consisting of a simple aqueous solution of neutral ammoniac tartrate and neutral sodic sulphate*. When portions of either of these fluids are boiled and poured into superheated flasks, they will continue quite clear for many days, or even for weeks—that is to say, although the short and rather narrow neck of the flask remains open the fluids will not become turbid, and no *Bacteria* are to be discovered when they are submitted to microscopical examination.

But in order to show that such fluids are still thoroughly favourable media for the multiplication of *Bacteria*, all that is necessary is to bring either of them into contact with a glass rod previously dipped into a fluid containing such organisms. In about thirty-six hours after this has been done (the temperature being about 80° F.), the fluid, which had hitherto remained clear, becomes quite turbid, and is found, on examination with the microscope, to be swarming with *Bacteria*†.

Facts of the same kind have also been shown by Dr. Burdon Sander-

* In the proportion of 10 grains of the former and 3 of the latter to 1 ounce of distilled water.

† The Modes of Origin of Lowest Organisms, 1871, pp. 30, 51.

son* to hold good for portions of boiled "Pasteur's solution." Air was even drawn through such a fluid daily for a time, and yet it continued free from *Bacteria*.

Evidence of this kind has already been widely accepted as justifying the conclusion that living *Bacteria* or their germs are either wholly absent from or, at most, only very sparingly distributed through the atmosphere. The danger of infection from the atmosphere having thus been got rid of and shown to be delusive, I am now able to bring forward other evidence tending to show that the first *Bacteria* which appear in many boiled infusions (when they subsequently undergo putrefactive changes) are evolved *de novo* in the fluids themselves. These experiments are moreover so simple, and may be so easily repeated, that the evidence which they are capable of supplying lies within the reach of all.

That boiling the experimental fluid destroys the life of any *Bacteria* or *Bacteria*-germs preexisting therein is now almost universally admitted; it may, moreover, be easily demonstrated. If a portion of "Pasteur's solution" be purposely infected with living *Bacteria* and subsequently boiled for two or three minutes, it will continue (if left in the same flask) clear for an indefinite period; whilst a similarly infected portion of the same fluid, not subsequently boiled, will rapidly become turbid. Precisely similar phenomena occur when we operate with the neutral fluid which I have previously mentioned; and yet M. Pasteur has ventured to assert that the germs of *Bacteria* are not destroyed in neutral or slightly alkaline fluids which have been merely raised to the boiling-point†.

Even M. Pasteur, however, admits that the germs of *Bacteria* and other allied organisms are killed in slightly acid fluids which have been boiled for a few minutes; so that there is a perfect unanimity of opinion (amongst those best qualified to judge) as to the destructive effects of a heat of 212° F. upon any *Bacteria* or *Bacteria*-germs which such fluids may contain.

Taking such a fluid, therefore, in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask with the assurance that it contains no living organisms. Having ascertained also by our previous experiments with the boiled saline fluids that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments. But when this is done, the previously clear turnip-infusion invariably becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria*.

Thus if we take two similar flasks, one of which contains a boiled "Pasteur's solution" and the other a boiled turnip-infusion, and if we

* Thirteenth Report of the Medical Officer of the Privy Council (1871), p. 59.

† How unwarrantable such a conclusion appears to be, I have elsewhere endeavoured to show. See 'Beginnings of Life,' 1872, vol. i. pp. 326-333, 372-399.

place them beneath the same bell-jar, it will be found that the first fluid remains clear and free from *Bacteria* for an indefinite period, whilst the second invariably becomes turbid in one or two days.

What is the explanation of these discordant results? We have a right to infer that all preexisting life has been destroyed in each of the fluids* ; we have proved also that such fluids are not usually infected by *Bacteria* derived from the air; in this very case, in fact, the putrescible saline fluid remains pure, although the organic infusion standing by its side rapidly putrifies. We can only infer, therefore, that whilst the boiled saline solution is quite incapable of engendering *Bacteria*†, such organisms are able to arise *de novo* in the boiled organic infusion.

Although this inference may be legitimately drawn from such experiments as I have here referred to, fortunately it is confirmed and strengthened by the labours of many investigators who have worked under the influence of much more stringent conditions, and in which closed vessels of various kinds have been employed‡.

Whilst we may therefore infer (1) that the putrefaction which occurs in many previously boiled fluids when exposed to the air is not due to a contamination by germs derived from the atmosphere, we have also the same right to conclude (2) that in many cases the first organisms which appear in such fluids have arisen *de novo*, rather than by any process of reproduction from preexisting forms of life.

Admitting, therefore, that *Bacteria* are ferments capable of initiating putrefactive changes, I am a firm believer also in the existence of not-living ferments under the influence of which putrefactive changes may be initiated in certain fluids—changes which are almost invariably accompanied by a new birth of living particles capable of rapidly developing into *Bacteria*.

II. "On Just Intonation in Music; with a description of a new Instrument for the easy control of all Systems of Tuning other than the ordinary equal Temperament." By R. H. M. BOSANQUET, Fellow of St. John's College, Oxford. Communicated by Professor H. J. S. SMITH, F.R.S., Savilian Professor of Geometry in the University of Oxford.

(Abstract.)

The object of this communication is to place the improved systems of

* [Note. Jan. 31, 1873.]—In 'The Beginnings of Life,' vol. i. p. 332, note 1, I have cited facts strongly tending to show that *Bacteria* are killed in infusions of turnip or of hay, when these have been heated to a temperature of 140° F. They also seem to die at the same temperature in solutions of ammoniac tartrate with sodic phosphate.

† See 'Beginnings of Life,' vol. ii. p. 35, and vol. i. p. 463.

‡ See a recent communication by Prof. Burdon Sanderson, in 'Nature,' January 9th.